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THE STRUCTURAL FAILURE OF THE LITHOSPHERE¹

As a foundation for ordinary human activities it is but natural that the lithosphere or solid earth should be a popular symbol of strength and permanence; but the geologist sees abundant evidences that it has fared badly in the contest with environmental forces, past and present. It has been weak and incompetent; it has bent, crumpled, broken and mashed; structurally it has failed; in considerable part it now consists of structural ruins.

The problem of the structural geologist includes the restoration of these ruins and a determination of the conditions and causes of failure. His problem is not rendered easier by the fact that it is seldom possible to see the structures in three dimensions, and that he must base his restoration on fragments of evidence seen at the surface or on the very limited outlook of underground openings or on inferences from environmental conditions. Furthermore, the geologist seldom sees rock failure in actual progress. If he does he may not recognize it because the movement is so slow. He arrives after the disturbance is over and must infer the nature of the forces and processes from the results. In attempting to picture conditions in the inaccessible deep zones, he must make long range inferences from the few available facts.

The study of structural geology naturally begins with the mapping and description of separate structures like folds, faults, joints, and cleavage. Too often this has been regarded as the end and not as a step toward the understanding of the structural conditions as a

¹ Address of the retiring vice-president and chairman of Section E, the American Association for Advancement of Science, Chicago, December 28, 1920.

whole. The necessity of integrating evidence and information from scant observations requires an understanding of the interrelations of structures and of great group characteristics of a given environment or of a given kind of rock. I would like to comment briefly on some of these broader considerations, not exhaustively, and certainly not with full understanding, but with a view to indicating some of the salient facts now known and the manner in which these facts have been built into certain generalizations and hypotheses as to movements of the lithosphere.

I. STRUCTURAL FAILURE IN THE ZONE OF OBSERVATION

We may direct our attention first to the structural failure of rocks extending downward only a few miles from the earth's surface. The characteristics of this region are disclosed to us by deformed rocks, some of which were once far below the surface, but now brought within our range of observation by the erosion of overlying rocks. This may be conveniently referred to as our zone of observation.

Heterogeneous Nature of Movement.—In this zone, some of the rocks have been deformed by rock flowage and some by rock fracture, both kinds of deformation often resulting in folding and tilting of beds. By rock flowage we mean "solid," "plastic," "massive," or "viscous" movement under great containing pressures during which the rock and its constituent minerals retain their properties of elasticity and rigidity. No one of these descriptive terms may be technically accurate and comprehensive, but the movement partakes of the characters expressed by all of them. The movement is not necessarily slow and continuous; there is geologic evidence that it is periodic. Rock flowage is essentially characterized by the parallel dimensional arrangement of minerals, like mica and hornblende, developed by recrystallization during the process. These minerals are present abundantly after the process, not before. Rock flowage is intimately associated with fracture, including the minute granula-

tion and slicing of mineral particles, and including larger fractures, especially of the shearing type. While rock flowage and rock fracture constitute two distinct types of deformation, there is almost complete gradation between the two, and much deformation is not accurately described by either term. A displacement may take place along a clean fracture, or along a fracture on which there has been local rock flowage, or along a zone of closely spaced parallel fractures with rock flowage affecting all of the intervening masses, or along a zone of rock flowage in which evidences of fracture planes are indistinct or altogether lacking. A single shear plane may show all of these features. In a large way a considerable zone of flowage may often be interpreted, in its relations to displacement and stresses, in much the same manner as a fracture plane.

The difficulty of a precise definition of the two phenomena of fracture and flow is well illustrated in the so-called flow accomplished experimentally. Shearing, thrust, granulation and slicing are here strongly in evidence, while the parallelism of mineral particles brought about through recrystallization, so conspicuous in schists, slates and gneisses, which are the principal evidence of rock flowage in nature, is almost lacking in the experimental results. Deformation induced artificially is plastic flow, but the same kind of deformation observed in nature is often called fracture. With a longer time factor the experimental flowage would presumably more closely approximate that of nature.

Structural failure within our zone of observation, whether by fracture or flow, has not been confined to any particular plane or formation, but is so distributed as to indicate that adjustment of rock masses under deforming stresses has been accomplished by movement in many zones, in many formations, in all directions, and with all inclinations. Rocks in this zone as a whole have not yielded to stresses as homogeneous masses. In fact, even down to comparatively small units of volume the rule is heterogeneity. No matter how homogeneous the formation may

seem, rock movement discloses zones of inherent weakness along which the movement is largely concentrated.

Causes of Movement.—Rock failure is evidence of overpowering stresses, but the causes and directions of these stresses are not so clear. Failure on a mountainous or continental scale points to great earth stresses of the kinds which have been variously ascribed to adjustments under gravity between earth masses of differing densities and topographic relief, to adjustments under gravity of a solid shell to a shrinking centrosphere, a conception based on the supposed transfer of heat and magmas from the centrosphere outward, to tidal strains, to changing centrifugal pressures caused by changes in rate of the earth's rotation, or to combinations of these causes.

So clear is the evidence that great earth forces of this kind have been operative that other causes of movement have been perhaps underestimated or ignored in explaining local failure. Such are the pressures and changes of temperature attending the extrusion and intrusion of igneous rocks, in the vicinity of which there is often clear evidence of local failure, the recrystallization of rocks during long periods causing local changes of volume, the leaching of substances near the surface causing voids and weakness and consequent slump under gravity, and other volume changes under weathering. When rocks are in a soft and incoherent condition, they are especially susceptible to local stresses. Mud, marl, sand and salt deposits crumple and slip as the deposits are slowly built up, either under air or water. Local loading by water and ice or rock materials may cause them to fail. Unconsolidated glacial deposits show a variety of joints, faults, and folds. In the settling, consolidation, and desiccation of soft deposits, stresses are set up resulting in local failure. When the deposits are seen later as hard rocks it is difficult to determine the extent to which the failures are to be attributed to these early and local causes acting during the soft formative stages, and to what extent they are the result of regional deformation after the rocks are strong and hard.

The part played by the forces of crystallization in initiating earth stresses is yet but little understood. Growing crystals have been found experimentally to exert considerable linear forces. There seems to be evidence in rocks that these forces have been sufficient to widen openings or to expand the rock mass. Crystallization may also contract the rock mass. The impressive manner in which crystal habit asserts and maintains itself under most intense conditions of metamorphism seems to indicate the reaction of considerable forces of crystallization to external environment. It is the custom usually to explain such facts on the basis of adaptation to environment, and to put the emphasis largely on the environmental conditions as determining the outcome. It is clear, however, that these conditions have not been sufficiently intense to interfere with or overcome the tendency of crystals to take whatever form best suits their atomic structure—in other words, to develop their own habit. The philosophy of the precise relations between inherent crystallizing power and environmental forces is not understood, but enough is known to warrant the suspicion that the cumulative effects of the forces of crystallization may themselves initiate earth stresses of a high order of magnitude.

In my own structural field work, I have become impressed with the necessity for better criteria for the separation of rock structures due to local causes of the kind above indicated from the results of failure under the greater regional earth stresses. Of course there is no clean-cut separation between the two. An accumulation of minor and local causes may cause relatively large earth movements, and conversely major earth movements are resolved into a complex of minor related structural phenomena.

Angular Relations of Rock Structures to Causal Stresses.—Just as structures in themselves do not indicate all the causes of failure, neither do they indicate clearly the directions of application of stress. On the whole the geologist's attempt to relate specific structures with specific stress systems has not been

highly successful. The various structures resulting from rock failure have usually been explained on the simple conception of the application of a non-rotational stress—either tension, causing elongation in the direction of pull, or simple compression, producing a shortening parallel to the principal stress and elongation at right angles to it. A fold, for instance, is assumed to indicate application of stress normal to its axial plane; a set of compressive joints is taken to indicate application of stress at 45° to the fractures; cleavage is taken to indicate application of pressure normal to its plane. Experimental work on rock deformation has been conducted mainly with the same limited assumptions, and the results have been widely quoted and applied to the interpretation of rock structures in the field. These conceptions may be correct as far as the immediate feature is concerned, but the forces are only minor constituents of the major causal movement and give no clue to its direction.

Much less attention has been paid to the conception that the compressional forces may be rotational, that is, that they may be applied in the form of a couple. Under this conception, the net result is a shearing between the heterogeneous rock units along planes ranging from parallel to 45° to the principal axis of stress, the shearing usually accompanied by local tension—in other words, no matter what the origin of compressional stresses and their angle of application, when applied to the heterogeneous rock masses constituting the earth they tend as a whole to act in couples and are resolved into components usually acting in directions inclined to the resulting planes of movement. A mountain making movement under this conception is a shear of certain rock masses over others, resulting in faults, joints, folds, and cleavage. Tensional stresses may be minor consequences of such shear. Field observations within the range of my own experience favor this view of the dominance of shear. It is the view also which geologists have commonly applied to an assumed shear of a thin brittle crust over a thin mobile zone below, though curi-

ously enough not to the local structures that can be observed.

Illustration of Shear Structures.—To illustrate the prevalence of shear structures: Most folds are not symmetrical and indicate by the inclination of their axial planes a drag of one structural unit past another. When this relation is conspicuous they may be called "drag folds." A fold has usually been regarded as indicating direct shortening normal to its axial plane, and therefore application of stress normal to this plane. The Appalachian folds, for instance, have been ordinarily discussed as indicating pressure from the northwest and southeast. The same results, however, can equally well be produced by a differential or shearing movement acting in directions inclined to the trend of the fold axes or to the mountain range as a whole. Experimental reproduction of Appalachian folds under shearing stresses gives more satisfactory results than experiments with normal shortening.² The folds indicate the direction of the shortening or elongation, in other words of the nature of the strain, but not the angle of application of the stress.

The interpretation of rock cleavage or schistosity, a common though not the only evidence of rock flowage, affords an especially good illustration of the danger of using narrow assumptions as to its relations to causal stresses. Cleavage is a capacity to part along parallel surfaces determined by the parallel dimensional arrangement of mineral particles. There is abundant proof that the schistose rock has been elongated parallel to the cleavage surface, and cleavage thus becomes evidence of elongation. It does not follow, however, that the stress producing elongation was applied normal to it.

The elongation may well have occurred under a shearing stress of the sort which exists when a mass of dough is rolled out on the table by the application of stress inclined to the table surface. Field studies of cleavage seem to indicate that in the majority of cases

² Mead, W. J., "Notes on the Mechanics of Geologic Structures," *Jour. Geol.*, Vol. 28, 1920, pp. 521-523.

the cleavage is merely the expression of the yielding of a weaker formation or weaker part of a formation by a slipping or differential movement between harder members. Even in areas with regional cleavage, the same interpretation may be applied on a large scale when the harder units in adjacent terranes are taken into account. My own observations in old pre-Cambrian terranes tend to the conclusion that cleavage, indicating rock flowage, has been confined to comparatively narrow mesh-like zones between large massifs. The evidence leading to this conclusion that cleavage is the result of slipping between rock masses may usually be checked by drag folds which develop simultaneously in the softer rocks, and by fissures and faults which develop simultaneously in the harder rocks.

The zones of movement marked by cleavage may have almost any inclination or direction, but the plane of the cleavage itself has a strong tendency toward steep inclination or verticality. Both in strike and dip the cleavage is more uniform than the movement zone of which it is a part. The relation is not unlike that between folds and cleavage, to be presently discussed. This steep inclination of cleavage does not necessarily indicate prevailing horizontality of stresses on the assumption that cleavage must develop normal to stress. In part it may have this relationship, but when considered in relation to folds and relative movement of adjacent massifs it more often indicates shearing stresses inclined to the cleavage. So far as any general inference is possible, the tendency of cleavage to show uniform strike and steep inclination over great areas suggests differential movement in vertical or steeply inclined planes, the movements in these planes ranging from vertical to horizontal. It can not be explained by movement along planes tangential to the earth, which would require prevalence of flat or gently inclined cleavage. In short the attitude of cleavage, so far as it may be generalized, does not correspond to the conception of the tangential shearing of a competent surface zone over a mobile zone below.

Cleavage has a definite relationship to folds which is of great usefulness in interpretation of rock structures, and which affords valuable suggestions as to the general relations of cleavage to the great zones of flowage of which it is often an expression. Cleavage is approximately parallel to the axial planes of the folds. It therefore usually stands more steeply than bedding and is more uniform in dip and strike than bedding. Where cleavage is noted in a rock outcrop, the direction and inclination of the axial planes of the folds are thereby indicated—not only for the folds within the rock observed, but also, usually, for the folds in the adjacent rocks as well.

As a consequence of the fact that cleavage is roughly parallel to the axial planes of folds, it follows that the trace of any bedding plane on the cleavage surface indicates approximately the direction and degree of pitch of the fold, that is, the inclination of the axial line of the fold to the horizontal. A single fragment of cleavable rock appearing in an outcrop may be sufficient to establish the pitch for a considerable area.

The inclination of bedding to cleavage—always remembering that the latter indicates the attitude of the axial plane of the fold—indicates faithfully the position of the observed bedding on the fold, whether the fold be upright inclined or overturned. This principle is useful in determining whether a bed is right side up or overturned. Inferences of the same sort may be drawn from strike observations on bedding and cleavage in deformed areas.

Still further, cleavage is a phenomenon of rock flowage. The very existence of cleavage, therefore, means the rock has been deformed under the conditions of rock flowage, where the folds are likely to be of a rather intricate type, with much interior thinning and thickening of the beds. Even though evidences of this folding are not immediately at hand, the very existence of cleavage on a considerable scale indicates with reasonable certainty the existence not only of folds, but folds of the rock flowage type.

All of these inferences may be made induc-

tively from a surprisingly narrow range of observation.

These remarks on cleavage apply to the structure ordinarily associated with the deformation of rocks which is almost without exception inclined to bedding or other primary structures. They do not apply to cleavage developed solely by load or gravity, which might reasonably be expected to be horizontal. The latter type of cleavage has been described for certain terranes and districts, as for instance in the Belt series of the Canadian boundary; but within my own observation of deformed areas it is a phenomenon of such local and special character as not to invalidate the generalizations above made. So far as load cleavage is assumed to develop under static conditions of load, without movement, I doubt its existence. Cleavage usually indicates movement, not static pressures.

The interpretation of jointing and faulting has likewise suffered from far too narrow and simple assumptions of the mechanical conditions. Quoting from a recent paper by Mead,³ such a simple structure as an open fissure or joint "obviously due to tensional stresses (so far as the fissure itself is concerned) may be an incident in simple elongation, shear, cross-bending, compression or shortening, or torsional warping. A reverse fault implies conditions of shortening or compression but may in addition to this possibly be an incident in a general shearing movement, or a phenomenon of simple cross bending, or may be due to torsional warping." In my own field of experience I have been impressed with the frequency of joints and faults developed as incidents in differential or shearing movements. There is rapidly accumulating evidence of the existence of great thrust faults with low dips as prominent features of diastrophism.

When the shearing movements have been determined by the study of a single type of structure like folds, important corroborative evidence may be obtained from other structures. Instead of regarding structures as independent units, each with its own set of mechanical conditions, they may be viewed as a group expres-

sion of some major movement. When so viewed the shearing nature of the movement often becomes obvious.

Distribution of Movements.—Within our zone of observation, it is difficult to say inductively whether or not there has been more movement or less movement with depth. Neither is it possible with any satisfactory degree of definiteness to discern controlling attitude or pattern in the complex of movement zones. The zones range from vertical to horizontal, are parallel or intersect. The original horizontal position of stratified rocks naturally suggests dominance of the horizontal element in movements affecting them, because of resolution of forces along bedding planes of weakness, but the beds soon become inclined or vertical when deformed and disturbed zones may be anything but horizontal. The less deformed masses between may have almost any shape. Locally they may be discoidal, or sheet-like, or oval or rod-shaped, or rhomboidal. Interesting attempts have been made to discern some controlling pattern, both in large and in small structural features, but subjective hypotheses enter to so large an extent that the reality of the pattern presented is often not convincing to others.

Possible Increase of Rock Flowage with Depth.—Within a few hundred or at most a few thousand feet of the surface, fracturing, much of it open, is clearly the dominant process, though even here soft rocks may yield by flowage. In the lower part of the zone of observation combined fracture and flowage is the rule. Fractures are more commonly of the closed shearing type. It has been easy to assume that this combination is merely transitional to a zone of flowage below. The fact that rocks which have been deeply buried are often highly schistose as a result of rock flowage has been cited as indicating increased rock flowage with depth. I have shared in this view. From some familiarity with ancient and formerly deeply buried terranes, I am not sure, however, but that a careful inductive study of field sections requires considerable qualifications of this generalization. Many instances may be cited of rock flowage occurring high

³ *Loc. cit.*, pp. 505-506.

in the geologic section and rock fracture below. On the whole, the oldest rocks undoubtedly show greater evidences of rock flowage, though even here such evidences are localized in relatively narrow and numerous zones. These rocks have suffered more periods of deformation, some near the surface and some deep below, than the younger rocks. The present evidences of flow do not necessarily indicate that all the flowage occurred at great depths. Plutonic intrusions of great mass often, not always, cause rock flowage in the adjacent beds, and so far as such intrusions are more numerous with depth, rock flowage may increase. On the other hand, some plutonic intrusions in younger series which have not been very deeply buried likewise cause rock flowage. Certain it is that shearing movements, resulting in displacements which we call faults, have extended down to the bottom of our zone of observation. These partake of the nature of rock fracture in their confinement to planes and in their relations to stresses, but whether the processes be called flow or fracture is partly a matter of definition to which we shall presently make further allusion.

II. THE UNSEEN ZONE BELOW

Below the zone where the evidences of structural failure can be observed, conceptions of the structural behavior of rocks are based on such a variety of assumptions that the layman, and for that matter the geologist, has much difficulty in understanding and reconciling the various views. It is certain that rocks fail in this zone; there is evidence which permits of no other conclusion; but the manner, distribution, and causes of this failure are by no means clear. There are certain fundamental facts upon which any hypothesis must be built.

Known Facts.—Tidal experiments have shown that the earth as a whole is stronger than steel and acts almost as an ideally rigid substance.

The behavior of earthquake waves indicates that the earth behaves as a solid throughout; and for the outer quarter of the earth, at least,

the waves increase in velocity of transmission with depth, showing that elasticity and rigidity increase faster than density.

Under surface conditions a dome of the strongest rock, corresponding to the sphericity of the earth, has a calculated supporting strength equal only to a very small fraction of the dome's own weight; but experimental work on deformation of rocks has shown that, with increase of containing pressures or cubical compression, the rock takes on a rigidity capable of resisting enormous stress differences. The range of experimental evidence is not yet sufficient to show the magnitude of these differential stresses necessary to produce deformation under the conditions of pressure which might be reasonably inferred below our zone of observation; but quoting from Adams⁴ "the experiments seem to indicate that with a containing pressure of about 10,000 atmospheres, which would be equivalent to a depth of about twenty-two miles below the surface, it would be impossible to make the marble flow, except under a pressure which would be simply colossal." Geologic evidence seems to indicate a supporting strength in the deep zone far greater than that of surface rocks.

The rocks in the deep zone are under higher temperature and greater pressure than in the zone of observation. Some notion of the quantitative values of these factors is afforded by downward extrapolation of observed gradients nearer the surface.

The density of rocks within the zone of observation averages about 2.7; the density of the earth as a whole as determined astronomically is in round number 5. It follows, therefore, that the density of part of the earth must be higher than 5, and that the density of the deep zone must be higher than at the surface; but beyond this the distribution of density in the deep zone, both vertically and horizontally, are unknown.

By means of the plumb line and pendulum,

⁴ Adams, Frank D. and Bancroft, J. Austen, "On the Amount of Internal Friction Developed in Rocks during Deformation and on the Relative Plasticity of Different Types of Rocks," *Jour. Geol.*, Vol. 25, 1917, p. 635.

it is known that the horizontal distribution of densities is heterogeneous. The density is low in the earth protuberances and high in earth depressions, as if the earth masses were in flotation equilibrium. The subcrustal densities are balanced against topographic relief. This is called isostatic equilibrium. Certain parts of the earth, called negative elements by Willis,⁵ seem to have been subjected during geologic history to long-continued deposition. Other parts, called positive elements, have been more commonly subjected to erosion than deposition. Negative elements are heavy and positive elements are light. Loading and unloading is not necessarily the primary cause of movement, but may serve to accentuate an inherent and prevailing tendency to isostatic adjustment between masses of differing density.

Isostatic balance is not complete. Some parts of the earth vary from this condition, suggesting that they have sufficient strength to sustain themselves in opposition to isostatic tendencies.

The observed relations between density and relief may be explained on the assumption that the differences in density extend uniformly to a depth of about 75 miles, called the depth of isostatic compensation. This figure is favored by geodesists. No one knows, however, the density gradients deep below the surface, or the extent to which there is heterogeneous vertical distribution of density. If instead of assuming the uniform downward extension of densities observed at the surface, assumptions are made of other vertical distributions of density, various other depths of compensation may be calculated, ranging up to several hundred miles. So far as geologic evidence goes, it seems to favor the view that depth of compensation is not uniform.

A comparison of the up-lift of mountain masses with their horizontal shortening indicates how deep the mountain making movements have extended.⁶ In general sharp close

⁵ Willis, Bailey, "Discoidal Structure of the Lithosphere," *Bull. Geol. Soc. of Am.*, Vol. 31, 1920, p. 277.

⁶ Chamberlin, R. T., "The Appalachian Folds of Central Pennsylvania," *Jour. Geol.*, Vol. 18, 1910,

folding indicates a comparatively shallow depth, whereas broad open folds, approaching the plateau type of deformation, can be explained only by movements of material extending to great depths. Major features of continental and oceanic relief also seem to require the latter inference. If the amount of shortening observed in some mountain ranges were to extend downward indefinitely, mountains much higher than those actually formed would have resulted; hence the conception of considerable movements of a shallow shell without equivalent movement below, and thus perhaps the conception of mobility of an intervening layer, though at widely different depths in different localities.

Geologic evidence points to periodicity in earth movements, indicating that the adjustment to stress is not uniform and continuous.

Finally, magmas originate well below our zone of observation and presumably take part in the mechanical easements. From the known conditions of rigidity already indicated, it seems certain that liquid condition is local and intermittent. Quoting from Gilbert:⁷

The continuous or secular relations of pressure, temperature, and density in the subterranean region from which liquid rock rises at intervals may be assumed to be such that moderate change of condition either induces liquefaction or else so lowers the density of rock already liquid as to render it eruptible; and such a balancing of conditions implies some sort of mobility.

From these facts it is clear that earth movements extend to considerable depths below our zone of observation, that the movements are periodic, that the earth as a whole is more rigid than steel when subjected to sudden stresses like earthquake shocks or tidal pulls, that it yields slowly and periodically to long continued stress; that as a whole it is sufficiently weak to allow a large measure of isostatic adjustment, but still strong enough to pp. 228-251; "The Building of the Colorado Rockies," *Jour. Geol.*, Vol. 27, 1919, pp. 145-164, 225-251.

⁷ Gilbert, G. K., "Interpretation of Anomalies of Gravity," Prof. Paper 85, U. S. Geol. Survey, 1914, p. 34.

support considerable structures against isostatic tendencies; that it is not essentially molten or fluidal in the ordinary sense; that molten magmas are probably local and incidental.

As to depth and distribution of the movements, and as to the manner of movement, whether by fracture or plastic flow or by some unknown process, there is wide divergence of opinion. Likewise, there is doubt as to the laws or control under which stresses may be transmitted. We may refer briefly to these questions.

Does a Zone of Weakness or Mobility Exist in the Unseen Depth?—A common conception of the distribution of movement deep below our zone of observation confines it to a single spherical zone of weakness or mobility surrounding the centrosphere and surrounded in turn by a rigid shell. This zone is supposed to be marked by a capacity to yield readily to long enduring strains. It may be in part the generating zone of magmas, which may be a factor in its supposed easy yielding. The conception of the existence of a weak and mobile zone has found expression in several ways.

The widely held belief in the existence of a zone of rock flowage below a surficial zone of fracture has commonly carried with it an assumption of the relative weakness and mobility of this zone. In fact "zone of rock flowage" and "zone of weakness" have come to be almost synonymous in discussion of this problem. Doubt as to this correlation is expressed later. Even if the existence of a single zone of rock flowage were proved, it does not necessarily follow that this is a zone of weakness.

Van Hise assigned a depth of only six miles to the top of this zone, though with the important reservation that increased rigidity under containing pressures would greatly increase this figure.

Adams and Bancroft,⁸ on the basis of experiments with rock failure under great containing pressures, conclude that the amount of tangential thrust required to produce movements increases so rapidly below the surface

"that the great movements of adjustment by rock flow or transference of material in the earth's crust from one point to another—other than the transference of rock in a molten condition—must take place comparatively near the surface," and that the ease of movement "increases rapidly in proportion to their nearness to the surface." The mobile zone thus implied is inferred from experimental results to be limited to depths within 35 miles, below which a condition of no mobility seems to be assumed.

Gilbert conceived "a relatively mobile layer separating a less mobile layer above from a nearly immobile nucleus," the mobile layer agreeing in depth with the depth of isostatic compensation.

Barrell called this weak zone the asthenosphere and assigned its provisional boundaries at depths of 75 and 800 miles from the surface. This he conceived to underlie the zone of isostatic compensation, which was calculated by Hayford to be 75 miles below the surface.

Hayford assumed concentration of movement within the lower part of the zone of isostatic compensation, that is within 75 miles of the surface.

Willis concludes that there is a zone of adjustment below 40 miles and extending to the base of the asthenosphere, and that the adjustments necessary to isostatic undertow take place mainly between 45 and 100 miles from the surface.

In contrast to these conceptions of a deep mobile zone, are the views of T. C. Chamberlin and R. T. Chamberlin, who postulate multiplicity and irregularity of movement zones.

R. T. Chamberlin⁹ concludes that mountain making diastrophism affects wedge shaped masses and implies steeply inclined zones of movement.

T. C. Chamberlin emphasizes the superficial nature of diastrophic movements of the mountain making kind, whether these are tangentially compressive or the result of creep of continental masses under gravity. In regard to deeper, so-called massive, movements of the

⁸ *Loc. cit.*, p. 635.

⁹ *Loc. cit.*

kind reflected in major features of continental and oceanic relief, he does not assume any mobile substratum, but rather steeply inclined zones of movement. As he states it:¹⁰ "Inherited inequalities of specific gravity are, perhaps more than any other agency, the governing power in shaping if not actuating diastrophic movements"—but that "the normal mode of isostatic adjustment in such an earth is thought to be wedging action in the form of movements on the part of its constituent tapering prisms, conical, pyramidal, or otherwise, in response to the varying stresses imposed on them. . . . They should reach to whatever depths may be seriously affected by differential stresses of an order requiring readjustment. No undertow in a hypothetical mobile substratum is necessarily involved and none is postulated."

These are only a few of the views that might be cited to indicate the wide range of hypotheses possible as to depth, number, and attitude of deep mobile zones. The very diversity of these views emphasizes the restricted range of known facts. The requirement of proof naturally rests most heavily on hypotheses which most precisely restrict the locus of movement. So many assumptions must enter into this proof that in our present state of knowledge it can not be rigorous. The safest scientific attitude for the time being would seem to be one of rigid adherence to the known facts, and the recognition of the possibility of more than one hypothesis to explain them. This is not incompatible with a sympathetic attitude toward the efforts of those attempting proof of a single hypothesis.

Until the time comes when it is possible to furnish definite proof of any specific localization of movement, my own inclination is to keep clearly in mind the distribution of movements within the zone of observation, already summarized, as perhaps the best guide to the condition that may be assumed at least for some distance below our lowest observations. This measuring stick is short, but there are

¹⁰ Chamberlin, T. C., "Diastrophism and the Formative Processes," *Jour. Geol.*, Vol. 21, 1913, p. 520; Vol. 26, 1918, p. 197.

some reasons for believing that it is as good as any yet available to measure our course through the complex of hypotheses possible in the deep zone. Especially is it desirable to keep in mind the fact that cleavage, indicating rock flowage, as observed in the deepest part of our zone of observation, does not in general have an attitude required by the conception of tangential shearing in a mobile zone. This does not disprove a different attitude below, but it does eliminate an affirmative bearing on the question which has been sometimes implied.

Are Deep Movements Accomplished by Rock Flowage Rather than by Rock Fracture?—It remains to consider the manner or processes through which deep movements are accomplished, whether by plastic flow, by fracture or by some combination of these kinds of deformation. The widely current hypothesis is that deformation in the deep zone is mainly by rock flowage. The deformed rocks have not been seen, nor have the environmental conditions been accurately measured; yet there are weighty considerations favoring this view:

Experimental work has shown that rock flowage requires containing pressures equal at least to the crushing strength of rocks, and these pressures surely exist in the deep zone. Within the zone of observation even the strongest rocks have locally suffered rock flowage and hence have locally, even at that shallow depth, been under containing pressures sufficiently in excess of their crushing strength to permit flowage. With greatly increased pressures at greater depths it is logical to argue that conditions for flowage would be improved. Under these conditions the resistance to deformation is a function of the internal friction or viscosity of the rock. This property does not of necessity bear any relation to the compressive strength or competency of the rock—qualities which determine its behavior in the absence of great containing pressures. Quartzite or granite, so far as we know, may have no greater viscosity than marble or slate. Adams' experiments show diabase and marble in a composite

specimen behaving similarly. In fact marble actually penetrated the harder diabase. Likewise, gypsum penetrated steel. While there are probably differences in the internal friction or viscosity of different rocks under these conditions, the results are nevertheless homogeneous in approximating rock flowage—in contrast to the heterogeneous results under less containing pressures where competency and strength of rocks play a part.

Earth temperatures increase with depth. Increase in temperature aids and accelerates rock flowage. This is evidenced by flowage of hard rocks at moderate depths at batholithic contacts. Also facts of physical chemistry show that increase of temperature increases molecular activity, hastens endothermic reactions (anamorphic reactions are largely endothermic), increases solution, both liquid and solid, and hence recrystallization, and decreases viscosity or internal friction.

Notwithstanding these and other considerations, any conclusions as to the existence of a deep zone in which all rocks flow when deformed is hypothesis, not proved fact, and perhaps will always remain so. The environmental conditions are not accurately known; and even if each of the factors were measured, their conjoint effect is still speculative. Variations in the time factor alone may determine whether a rock flows or fractures. Rock flowage which has occurred in rocks now accessible to our observation fails to indicate increase with depth with sufficient clearness and definiteness to warrant confident downward projection.

Experimental evidence has been construed to indicate that under great containing pressures, of the kind probably existing at depth, the movement under thrust or shear is of the nature of rock flowage, but this is partly a matter of definition. The rock breaks and granulates, often along definite planes, but the parts are still held together; it really flows. Displacements along these planes may partake of the nature of faults, and there is no development of true flow cleavage determined by a parallel arrangement of minerals under recrystallization, the common geologic

evidence of rock flow. Presumably with longer time and proper conditions of temperature and mineralizers, parallelism of newly developed minerals, characteristic of rock flow, would result. So far as the experimental results go, however, they fail to exhibit structures which in ordinary geologic field interpretation would be classed as *typical* rock flowage. They would be called fracture or combined fracture and flowage. They would be described as shear planes and faults. They might suggest rupture of the kind that originates earthquake shocks.

Rock flowage has been widely assumed to indicate weakness and mobility. The correlation of rock flowage with weakness may arise from the fact that certain soft rocks such as shales, which are inherently weak, may often be observed to have undergone rock flowage, while adjacent strong rocks have been unaffected. Or, a zone of flowage passing through a homogeneous formation unquestionably indicates movement along the flowage zone, and, therefore, indicates the weakness of this zone relative to adjacent undeformed parts of the mass. But it would be equally valid to argue that where fracturing has been concentrated along a zone between undeformed rocks it too indicates movement, and therefore relative weakness. It is a long step from this to the conclusion that flowage indicates greater weakness than fracture. It is entirely conceivable that it might require more energy to make rock flow than to make it fracture. Indeed there is some reason for believing, both from experimental work and from observations in areas of combined fracture and flowage, that relief actually takes place first and most easily by fracture and that flowage occurs only when it is possible to concentrate much more energy into the rock. Both structures show weakness relative to adjacent undeformed masses, but in relation to each other degree of weakness is a much more complicated problem.

Our question, then, as to the extent to which deep movements are accomplished by rock flowage can not be simply and definitely answered in the present state of knowledge.

The preponderance of environmental evidence seems to indicate that rock flowage is the distinctive kind of movement, but so many qualifications, definitions and assumptions enter into this conclusion that my present inclination is to keep firmly in mind the complex facts of deformation in our zone of observation as a possible key to the interpretation of unseen movements. This attitude will require us to pay more attention than heretofore to the possibilities of heterogeneous structural behavior at great depths. Particularly should we keep in mind the fact that the kind of rock flowage accomplished experimentally produces structures which in the earth have sometimes been called fracture or combined fracture and flowage. We may assume a downward extension of combined fracture and flowage, as observed in the field, and still meet the conditions of flow implied by experiment.

How Are Stresses Transmitted in the Deep Zone?—In our zone of observation stresses are clearly transmitted by the competent members of the lithosphere. In any area of deformation evidence may usually be found of the control of the structure by one or more competent members. When the notion was widely held that the interior of the earth was molten or fluidal, hydrostatic stress conditions were naturally assumed. With the later knowledge that the earth acts essentially as a solid throughout, this view was largely abandoned in favor of the view that rocks in the deep zone act as rigid competent members capable of transmitting stresses in definite directions. The vector properties of cleavage and other structures supposed to develop in this zone were cited to indicate the definite orientation of stresses. It does not follow from this, however, that pressure conditions were or are not hydrostatic, especially under slow movements. Rocks under compression from all sides greater than their crushing strength seem to transmit stresses in a manner suggesting approach to hydrostatic conditions of pressure. When the stress differ-

ences are such as to require it, there is movement in the direction of easiest relief. The stress as reflected by the movement would seem to have been transmitted in a definite direction, and yet the pressures may have remained hydrostatic. If we were to imagine a volume of liquid deep below the surface subjected to differential stress sufficient to deform its containing walls, it is clear that the movement would be in the direction of easiest relief, notwithstanding the hydrostatic conditions within the liquid. Periodicity of movement is possible under this conception. Rock structures indicate movement only, not necessarily the inherent stresses. Movement of rocks under the conditions supposed to obtain deep below the surface seems likely to be at least in part a matter of relief of materials so contained between rigid members that the direction of escape is definitely oriented. Of course this supposition assumes that on some scale, small or large, there are units of mass competent to act as retaining walls for materials acting under hydrostatic pressure. If all the mass in the deep zone were under hydrostatic pressure, the retaining walls might be regarded as the solid shell above, inequalities in the competence of which would control the movements in the direction of easiest relief. However, rock structures, such as cleavage and folds, with vector arrangement of the sort observed near the surface and of the sort supposed to exist below, tell us only of the direction of movement and fail to indicate whether the stresses are hydrostatic or otherwise.

CONCLUSION

Within the zone accessible to observation movements of rock masses are accomplished by fracture and flowage. These processes may be distinct and separate, or so interrelated as to make definition difficult. The zones of movement are many, their positions and attitudes diverse. In general they indicate shearing or grinding movements between rock masses, accomplished both by fracture and flowage, and caused by stresses inclined to

the zones of movement. This conception is taken to afford the best initial basis for the interpretation and correlation of observed rock structures. There is no certain evidence of increase or decrease of movement toward the bottom of this zone. Beyond a shallow surface zone, there is no certain evidence of increase of rock flowage and decrease of rock fracture with depth. There is no certain evidence that rock flowage means greater weakness than rock fracture. There is no certain evidence in rock flowage that pressures are dominantly hydrostatic or dominantly those of competent solid bodies.

Movements are known to occur in the zone below our range of observation, but their nature and distribution are the subjects of varied hypotheses based on a few known conditions. Much of the sharper diastrophism seems to be confined to a thin surficial zone. Deeper movements, of a more massive type, periodic, and possibly slower, seem to be implied by the relative movement of great earth segments as represented by continents and ocean basins. Their depth is unknown. Most of the current hypotheses agree in assuming a single mobile zone in which rocks move dominantly by rock flowage. The basic requirements of reasonable hypothesis, however, may be equally well met by a conception of movement much like that of the zone of observation. This does not require or postulate the conception of the existence of any single mobile zone, or zone of slipping, or zone of flowage, or of an asthenosphere. It supposes movement irregularly distributed in many zones, with any inclination, and accomplished by both fracture and flowage as far below the surface as movement extends—always remembering that some of the structures geologically described as fractures, may be expressions of mass movement of the kind defined as flow in experimental results.

Conditions of temperature and pressure and vulcanism become more intense with depth, but it remains to be shown that their conjoint action results in a uniform environ-

ment, and even if it does, that this condition is not upset by what might be called a heterogeneity of the time factor as represented by differing rates of deformation. If homogeneous environmental and time conditions are assumed, it is yet to be shown that these are sufficient to overcome the heterogeneity of the physical properties of the rocks and to cause homogeneous behavior through any considerable zone. It is not even certain that they may not fix and accentuate the heterogeneous properties of rocks. Certainly in the zone of observation there is comparatively slight evidence of their efficacy in causing more uniform deformation with depth.

In short, as between alternative conceptions as to the conditions in the deep zone, the burden of producing affirmative evidence would seem to rest heavily on any conception involving radical departure from the known irregular distribution and manner of movement within our zone of observation. We come, therefore, to the Chamberlin conception of a heterogeneous structural behavior of the earth.

C. K. LEITH

UNIVERSITY OF WISCONSIN

SCIENTIFIC EVENTS

DINNER IN HONOR OF THE RETIRING SECRETARY OF AGRICULTURE

THE success of Secretary E. T. Meredith in interesting the public in the investigational work of the U. S. Department of Agriculture has been unique. His prompt recognition of the needs of the department and his activity in behalf of the investigators there, have attracted the attention of scientific men throughout the country. Coming to the secretaryship at a time when the morale of the scientists in many government departments was being seriously impaired through discouragement as to the possibility of securing adequate support for investigation, his campaign of education had the effect both of awakening the public to the extent and importance of the work, and of heartening the workers.

It was then appropriate that before his retirement, there should be some demonstration of appreciation by the scientists themselves. This took the form of a buffet supper at the Raleigh Hotel, Washington, February 16. The event was planned by a committee chosen from the membership of the various Washington scientific societies in which the Department of Agriculture is largely represented. In the menu were included various items representative of the work of the department, such as "Dasheen Chips," "Soy Bean Sauce," "American Roquefort Cheese," and "New Unnamed Grapes." During the evening, Dr. B. T. Galloway presented Secretary Meredith with a vellum volume bound in hand-tooled, dark morocco, and containing the following inscription of appreciation signed by the five hundred and sixty scientific and technical men who attended:

The researches of the United States Department of Agriculture in recent years have become so diversified and so important for the welfare of the country and are so absolutely dependent on a wise, far-seeing and sympathetic administration, such as you have given us, that your departure from among us is a matter of very general regret.

Your broad comprehension and appreciation of the fundamental importance of scientific research in agriculture, your prompt recognition of the needs of the service and your enthusiasm and effective efforts to secure proper recognition of the work and workers have been most stimulating to us and have been of the greatest value in promoting a better understanding of the activities and purposes of the department and their vital relation to the business and industrial interests of the nation and the progress of the whole people.

In view of the above facts, we the undersigned, desire to express our deep appreciation and to thank you for what you have done and extend to you our hearty good wishes for all time to come.

In response, the secretary spoke briefly of his interest in the scientific work of the department, and his hopes for its future development. The esteem in which Mr. Meredith is held, was evidenced by the large attendance at this unofficial gathering. And the spirit of those present was such that when all joined in a rousing cheer for "Meredith" and

in singing "He's a jolly good fellow" it seemed not only wholly in harmony with the occasion, but a fitting expression of their enthusiasm for the man.

CONGRESS ON MEDICAL EDUCATION

THE Annual Congress on Medical Education, Licensure, Hospitals and Public Health will be held at Chicago on March 7, 8, 9 and 10, under the auspices of The Council on Medical Education and Hospitals, and The Council on Health and Public Instruction of the American Medical Association, The Association of American Medical Colleges, The Federation of State Medical Boards of the United States and The American Conference on Hospital Service.

The program of the sessions on Medical Education are as follows:

Introductory Remarks, Arthur Dean Bevan, chairman of the Council on Medical Education and Hospitals, Chicago.

The Significance of Group Practice in its Relation to the Profession and the Community, Veader N. Leonard, Academy of Clinical Medicine, Duluth.

SYMPOSIUM ON GRADUATE TRAINING IN THE VARIOUS MEDICAL SPECIALTIES

Medicine and the Medical Specialties—

- (a) Internal medicine, George Blumer, clinical professor of medicine, Yale University.
- (b) Pediatrics, Harry M. McClanahan, professor of pediatrics, University of Nebraska.
- (c) Nervous and mental diseases, Arthur S. Hamilton, professor of nervous and mental diseases, University of Minnesota.
- (d) Dermatology and syphilology, William A. Pusey, emeritus professor of dermatology, University of Illinois.

Surgery and the Surgical Specialties—

- (a) Surgery, Charles H. Frazier, professor of clinical surgery, University of Pennsylvania.
- (b) Ophthalmology, Walter B. Lancaster, Boston.
- (c) Oto-Laryngology, Wendell C. Phillips, New York.
- (d) Orthopedic surgery, Robert W. Lovett, professor of orthopedic surgery, Harvard University.

(c) Urology, Hugh H. Young, clinical professor of urology, Johns Hopkins University.

The Relation of the General Practitioner to the Specialist, James B. Herrick, professor of medicine, Rush Medical College.

Obstetrics and Gynecology, J. Whitridge Williams, dean and professor of obstetrics, Johns Hopkins University.

Public Health and Hygiene, Victor C. Vaughan, dean and professor of hygiene and physiological chemistry, University of Michigan.

Preclinical Subjects—

(a) Anatomy, Albert C. Eycleshymer, dean and professor of anatomy, University of Illinois.

(b) Physiology, Joseph Erlanger, professor of physiology, Washington University.

(c) Pharmacology and therapeutics, Charles W. Edmunds, professor of materia medica and therapeutics, University of Michigan.

(d) Pathology and bacteriology, James Ewing, professor of pathology, Cornell University.

Summary of Reports on Graduate Training in the Specialties, Louis B. Wilson, chairman of the Council's Committee on Graduate Medical Education, Rochester, Minn.

THE MANUFACTURE OF CHEMICALS FOR RESEARCH WORK

To reduce the cost of chemicals needed for research work in various scientific departments of the University of Wisconsin, the chemistry department will give a new course in the manufacture of organic chemicals during the summer session under the direction of Professor Glenn S. Skinner. The only other course of this kind given anywhere in the country is at the University of Illinois.

Professor J. H. Mathews states that most of the chemicals now available for experimental work are obtained only at excessively high prices and the department is compelled to make the choice between excessively high laboratory fees or curtailment of laboratory instruction. It will be possible with the laboratory facilities available during the summer months to manufacture these chemicals more cheaply than they can be purchased, thus materially cheapening the cost to the student.

All men of science in the university have

been asked to leave their orders for chemicals with Professor Skinner and as far as is possible these orders will be filled by his course.

Only eight advanced students will be admitted to the course, and they will work from nine to ten hours a day and will receive about 40 cents an hour for their work. Only the most promising graduates and upper classmen will be selected for the work, with the view to giving them intensive training in practical organic chemistry and experience in larger scale operations.

INSTITUTE FOR FOOD RESEARCH AT STANFORD UNIVERSITY

THE Carnegie Corporation of New York announces that it has entered into an agreement with Leland Stanford Jr. University, by which a food research institute is to be established at the university for the intensive study of the problems of production, distribution and consumption of food. The corporation expressed hope that the new organization will in time be known as the Hoover Institute.

Need for such an institution was first suggested to the corporation by Mr. Herbert Hoover, former food administrator and a trustee of Stanford University. The selection of Stanford was due in part to the fact that there is deposited there documentary material relative to the economic side of the war gathered by Mr. Hoover. He will serve as a member of the advisory committee.

The institute will begin work July 1. The corporation will provide \$700,000 for its support for ten years.

The university has agreed to make its scientific laboratories available to the institute. It is not intended to duplicate the equipment of research laboratories working in the field of nutrition, but to cooperate with other agencies.

Need for continual research work in problems arising after food has left the farmer's hands was emphasized by experience during the war, it is explained, when the study of food supply was necessary to attain maximum efficiency in the nutrition of the nations involved. During the war much of the previous data regarding food was found to be inaccurate. It

now is hoped to eliminate waste through scientific research.

Under the terms of the agreement Leland Stanford will appoint three scientific men, with authority to determine policies and problems to be studied. There also will be an advisory committee of men of national prominence, representing agriculturists, consumers, business men and other groups. The university will appoint seven members of this body to serve with the president of the university and the president of Carnegie Corporation, *ex officio*, for a term of three years.

SCIENTIFIC NOTES AND NEWS

THE Bruce gold medal of the Astronomical Society of the Pacific has been awarded for the year 1921 to M. Henri Alexandre Deslandres, director of the Astrophysical Observatory of Meudon, France, for his "distinguished services to astronomy."

PROFESSOR JULES BORDET, to whom the Nobel prize in medicine was recently awarded, has been elected a member of the senate of Belgium from the Hainaut district.

WE learn from *Nature* that at a meeting of the award committee, consisting of the presidents of the principal British engineering institutions, the first triennial award of the Kelvin gold medal was made to Dr. W. C. Unwin, who was, in the opinion of the committee, the most worthy to receive this recognition of pre-eminence in the branches of engineering with which Lord Kelvin's scientific work and researches were closely identified. The Kelvin gold medal was established in 1914 as part of a memorial to the late Lord Kelvin and in association with the window placed in Westminster Abbey in his memory by British and American engineers.

GEORGE C. WHIPPLE, professor of sanitary engineering in the Harvard Engineering School, has been elected a fellow in the Royal Institute of Public Health.

THE Medical Society of the City and County of Denver has appointed a committee to plan a meeting in appreciation of Dr. Hubert Work,

Pueblo, the president-elect of the American Medical Association.

DR. J. M. ALDRICH, of the U. S. National Museum, was elected president of the Entomological Society of America at the Chicago meeting.

PROFESSOR GEORGE A. DEAN, of the Kansas State Agricultural College, was elected president of the American Association of Economic Entomologists at its recent annual meeting in Chicago.

DR. W. R. G. ATKINS, of Trinity College, Dublin, has been appointed head of the department of general physiology at the Plymouth Laboratory of the Marine Biological Association.

WE learn from the *Journal* of the Washington Academy of Sciences that Mr. W. F. Wallis, of the department of terrestrial magnetism, Carnegie Institution of Washington, left Washington on January 9 for Huancayo, Peru, where he will succeed Dr. Harry M. W. Edmonds as magnetician-in-charge of the Huancayo Magnetic Observatory upon the conclusion of the latter's two-year assignment. Dr. Edmonds will return about April *via* San Francisco for duty at Washington.

DR. H. L. SHANTZ has been appointed plant physiologist in charge of plant physiological and fermentation investigations in the Bureau of Plant Industry. Dr. Shantz returned in September from a year's trip through Africa for the Office of Foreign Seed and Plant Introduction.

MR. A. D. WILSON, who has been director of agricultural work for the University of Minnesota for the past twelve years and superintendent of Farmers' Institutes for the State of Minnesota for the past fourteen years, has resigned these positions to take up farming in northern Minnesota, the resignation being effective on June 30.

MR. W. H. KENETY, who has been assistant professor of forestry in the University of Minnesota and superintendent of the Forest Experiment Station at Cloquet for the past eight years, has resigned to take a position

with a commercial wood products utilization company.

DR. EDWARD A. SPITZKA assumed his new work in the neuro-psychiatric section, medical division, War Risk Insurance Bureau, Washington, D. C., on March 1.

DR. HORACE W. FRINK, assistant professor of neurology at the Cornell Medical College, has sailed to work in psycho-analysis with Professor Freud at Vienna.

PROFESSOR SELSKAR M. GUNN, formerly associate professor of public health at the Massachusetts Institute of Technology, who has served for three years as associate director of the Commission for the Prevention of Tuberculosis in France, has left for Prague, Czechoslovakia, where he is to act as adviser in Public Health to the Ministry of Public Health. This appointment is in connection with the program of cooperation between the International Health Board of the Rockefeller Foundation and the Ministry of Public Health.

A MEMORIAL lecture on the life and work of the late Sir William Abney will be delivered before the Royal Photographic Society of Great Britain by Mr. Chapman Jones.

As a tribute to the services and character of the late General William C. Gorgas, the Senate has ordered that the remarks made at the memorial services in his honor, held at Washington, D. C., January 16, be printed.

UNIVERSITY AND EDUCATIONAL NEWS

THE sum of \$1,000,000 has been given to the new School of Medicine and Dentistry of the University of Rochester, by Mrs. Gertrude Strong Achilles and Mrs. Helen Strong Carter, daughters of Henry A. Strong, who died in Rochester in 1919. The money will be used toward the erection of a clinical hospital as a memorial to the father and mother of the donors.

THE *Bulletin* of the American Mathematical Society announces that in the department of mathematics at the University of Illinois, As-

sociate Professor R. D. Carmichael has been promoted to a full professorship; Dr. C. F. Green, Dr. L. L. Steimley, and Dr. B. Margaret Turner have been appointed instructors; Professor E. R. Smith, on leave of absence from Pennsylvania State College, has been appointed associate.

DR. RHODA ERDMANN, formerly lecturer at Yale University, has been appointed lecturer on experimental cytology in the University of Berlin.

At the University of Cambridge Dr. W. L. H. Duckworth, Jesus College, has been appointed to the newly created readership of anatomy, Mr. F. A. Potts, Trinity Hall, demonstrator of comparative anatomy, V. C. Pennell, Pembroke College, an additional junior demonstrator in anatomy and Dr. C. S. Myers, Gonville and Caius College, has been appointed reader in experimental psychology.

DISCUSSION AND CORRESPONDENCE

HUMAN NATURE AS A REPEATING FACTOR: THAT THRICE TOLD TALE

THE following comments on Professor Wood's "Thrice Told Tale," *SCIENCE*, January 14, 1921, are based upon my long experience in showing celestial objects through a great telescope to tens of thousands of Saturday-night visitors, and in explaining photographs of star clusters, the Milky Way, spiral nebulae, etc., to thousands of others. Perhaps these comments will be of interest to the psychologists.

I fear that Professor Wood is unduly concerned about the victimization of present-day expositors of the universe, including himself. Contrary to his implication that the response to his (Wood's) explanation of the universe, made by the chance visitor to his ingenious telescope, could never be made again, I would say that the incident in all its essentials has certainly happened many times, and it will doubtless occur many times in the future, for human nature is a first-class repeating factor.

When visitors to an observatory get a sudden appreciation of the bigness of our sun and other stars, of the number of suns in our stellar system, of the possible number of

planets revolving around those suns, of the strong probability that intelligent life exists in abundance throughout the universe, of the number of the spiral nebulae, of the probable sizes and masses of the spirals, etc., they frequently react with the comment that, if what the astronomer says (of the universe) is true, it doesn't matter much whether we (the people of the nation or the peoples of the earth) do this or do that. Their "this" and their "that" are generally dictated by the subject which happens to be uppermost in the public mind at the time. If our country is thoroughly interested in the presidential campaign, as it certainly was in the struggle of June, 1912, what is more natural than that Professor Wood's lone visitor should not be the only person to illustrate his philosophy by turning to that absorbing question of the day? And so, following a sudden comprehension of the extent and contents of the universe, our Hercules cluster visitor reacted, "I think it doesn't matter very much whether Roosevelt or Taft is nominated at the Chicago convention;" and G. Lowes Dickinson's lone telegraph operator in a railroad shack in the Rockies reacted, "I guess it doesn't matter two cents after all who gets elected president."

Other visitorial reactions here have drawn upon other subjects occupying the public mind, but there is no call to describe them now.

I recently asked one of my colleagues who has dealt extensively with the visiting public in the past twenty-six years whether he has had any experience bearing on this subject. He replied: "I have on several occasions drawn visitors' responses paralleling the incident described in your address. I have observed this reaction, not only in connection with visitors to the observatory, but from members of audiences to which I have lectured. Last month I delivered a short lecture to the patients in the tubercular hospital at Livermore, California, on 'Life in other worlds,' making references to the great number of suns in our stellar system, the possible multitudes of planets revolving about those suns, and the probability that many of those planets are inhabited. At the close of the lecture one of the patients came

up to me and said, 'After listening to your lecture, I don't think it matters much whether we patients get well or not.'"

I am respecting the value of understatement in saying that the essential parts of Professor Wood's story have happened here many times in the past thirty-three years in connection with the more than 200,000 visitors whose ideas of the universe have been enlarged in an immense number of cases by looking through the telescopes or by listening to the interpretation of astronomical photographs. I hope it is also an understatement to say that my experience in dealing with the public along this interesting psychological line seems to have been somewhat more extensive than that of others who have written on the same subject.

May I turn from these natural happenings to an incident truly astonishing? In some well-known book I have read of a human being who, looking at the moon through a telescope, was told that the large ring-formation in view was the crater Copernicus (or possibly Tycho or Archimedes—I can not locate the passage now), and who said to his instructor, "I should like to know how astronomers discovered that the name of that crater is Copernicus." This imaginary event is widely known in astronomical circles, but no one, in my opinion, had thought that it actually happened or even could happen. Yet, one Saturday night in the nineties a visitor descending from the observing chair said to me in all seriousness and innocence, "I was able to follow your description of the moon's surface, but I should like to have you tell me how astronomers discovered that the name of that large crater is Copernicus." If this unnatural incident could repeat, why waste energy and ink over the hypothesis that Wood's neighbor, acting in accord with widely prevailing philosophy, was a genuine unique?

W. W. CAMPBELL

MOUNT HAMILTON, CALIFORNIA,
February 17, 1921

GALILEO AND WOOD

TO THE EDITOR OF SCIENCE: I have long been interested in horns, and I should dearly

like to blow a blast on a David Wilbur Horn. To him I will say merely "*Quis custodiet ipsos custodes?*" Let the chemist take heed when murdering romance lest he also murder Cicero. I beg to associate myself with that veteran story-teller, T. C. Mendenhall, whose stories were so good that it never occurred to any one to doubt them.

I will take a little whack at the Galileo story myself, after relating my experience with the Wood story. In the summer of 1912 I was on the train going from London "up" to Cambridge with the guests for the quarter millenium of the Royal Society when I heard Dr. Nicholas Murray Butler telling it to Sir Oliver Lodge, and I assisted him, as Professor Wood had told it to me several years before as having happened at Easthampton. What was my surprise then at seeing Professor Campbell's account as happening later at the Lick Observatory! I immediately wrote him and Professor Wood. In my opinion Wood's story is the better, but I never could believe that the definition in that revolving mercury paraboloid could be good enough for a farmer to make such an observation. I always felt that this telescope in the well was one of Professor Wood's jokes. It was particularly wooden. Perhaps Professor Wood will pardon me if I insert some lines that I wrote in his guest book expressing my feelings on the subject. It will easily be seen that I am no great poet.

Ding, dong, bell,
Prof is in the well.
What did he put in?
Lots of time and tin.¹
What did he get out?
Nothing, just about.
What a silly prof was that,
He never knew what he was at.

I am bound to admit that the Royal Society did not agree with me when they elected him a foreign member.

As for Galileo, some years ago I was invited to deliver an address at the dedication of a new physical laboratory at a great university not a thousand miles from here. Sup-

¹ Poetic for mercury.

posing I was to be "the big noise" I prepared an address about an hour long, but was somewhat disconcerted on being introduced by the dean in an address of about half an hour, in which much of the wind was taken out of my sails. In it he used the words, "When Galileo dropped the two weights from the tower of Pisa he sounded the death-knell of the Aristotelian philosophy." Singularly enough the same sentence occurred in my address. But I had my revenge. In beginning I disclaimed all possibility of thought-transference, and when I came to the quoted words I added "as Sir Oliver Lodge says." I was rewarded with roars of laughter, and when I arrived at the club was told that the joke was much appreciated, as the dean was not popular. The joke would have been on me, however, if my manuscript had been looked at, for no more than the dean had I given Lodge credit for the remark that we both had cribbed. He laughs best who laughs last, for the dean is now president of that great university, while the subscriber is even less of a noise that he was then. However, hurrah for history! was it Napoleon who called it "*mensonges convenus*"?

ARTHUR GORDON WEBSTER

WORCESTER, MASS.,

February 13

ARCHEOLOGICAL SPECIMENS FOR MUSEUMS

THE curator of the Museum at Phillips Academy has received authority from the trustees to reduce the number of specimens possessed by the department of archeology. We have large numbers of various objects in stone, bone and clay, found during the course of our explorations in New England, the Middle West and the South. We propose assembling collections ranging from 500 to as high as 4,000 specimens, all recorded as to locality from our catalogue, etc., and to send these to museums, natural history societies, etc. There is no condition, but it is requested that certain of the specimens be exhibited. They will be found of value to students. These exhibits have cost us a great deal to accumulate, and while we ask no financial

return, we feel that those who receive the collections should pay the expenses of cataloguing, assembling, packing and shipping. The smaller collections will require several days to prepare and ship, the larger ones one or two weeks. The cost of clerical and other assistance will range from \$65 to \$200, depending on the size of the collection.

W. K. MOOREHEAD,
Curator

ANDOVER, MASS.

PUBLICATIONS OF THE VIENNA MUSEUM

DR. VICTOR PIETSCHMANN, as successor of the late Dr. Steindachner, writes of the sad plight of the museum of Vienna in having no means for publication, and no means of disposing of two works already printed. One of these is a Monograph of the Genus *Tenthredo*, the other a Monograph of the *Siphonæ Verticillatæ* from the Carboniferous to the Cretaceous with plates, by Dr. J. Pia. This great work on fossil plants is said to be of especial value, and Dr. Pietschmann has great hopes that some one in America may take fifty copies at \$5.00 each. The price is not great and the crisis is pressing. I suggest that any one willing to help this great center of scientific work to rise to its feet, may (as I have done) send a check for the equivalent in Kroner of five dollars to Dr. Pietschmann, Mechelgasse 2, Vienna 111.3.

DAVID STARR JORDAN

QUOTATIONS

THE PROTECTION OF BRITISH OPTICAL INDUSTRIES

THERE are two main objects which the Bill to be introduced should secure and reconcile. On the one hand, if the industry is to be saved, the manufacturers must be protected from foreign competition aggravated by the state of the exchange; and, on the other, the users of scientific instruments must not be prejudiced or hampered, either by being unable to obtain the best instruments or by having to pay an extravagant price for them. These apparently conflicting interests are not merely recon-

cilable; they are interdependent. If the British optical industry should dwindle and die, the scientific users of instruments will be at the mercy of foreign manufacturers, they will have to pay a heavy price for such dependence, and they will be handicapped as compared with scientific workers in foreign countries possessing a flourishing scientific instrument industry. Similarly, if the scientific users can not obtain the best instruments for their work, or if they have to pay an exorbitant price for them, their work will be hampered, their demand for instruments will decrease, and the manufacturers will ultimately suffer.

The industries, through the British Optical Instrument Manufacturers' Association, ask shortly for the following measures of protection:

1. No optical glass or scientific instruments to be imported into this country for a period of, say, seven years, except under license.
2. Such licenses only to be granted in respect of goods which are not being made in Great Britain in the required quantities or of the required quality.
3. An expert licensing committee to be set up.
4. The optical instrument manufacturers are prepared, in order to guarantee reasonable prices, to submit to a control of profits.

The manufacturers are satisfied and confident that, under such conditions for a limited period, they would be able to establish the optical glass and optical instrument industries on a sound and stable basis, and also be able at the end of the period to meet any foreign competition in the open market. On the other hand, unless they secure this limited protection, it is more than probable—indeed, it is almost certain—that the manufacture of optical glass in this country will cease, and that, in consequence, some of the largest British manufacturers of optical instruments will greatly curtail their production. The proposed measures seem to protect adequately the interests of the scientific users. Moreover,

such a system of control of imports for a limited period seems preferable to anything in the nature of a permanent tariff. It is not likely to have on the industry the emasculating effect of a protective tariff; provided that the period be limited, and that the licensing committee adopt an enlightened policy, prohibition of imports, except under license, is rather calculated to act as a stimulus on the development of the industry.

There is, finally, one point not dealt with in the proposals outlined above. In return for this shield from danger during a limited period, the country may well ask: What guarantee is there that the manufacturers are taking due measure to promote and prosecute the scientific research and scientific methods on which alone ultimately these, or any other, industries can be made efficient and able to stand against foreign competition? The leading manufacturers have combined to form a scientific instrument research association, and in addition many of them are engaged continuously in scientific research. But it is not clear that all the manufacturers who are demanding the legislative measures outlined above are contributing in either or both of these ways to the advancement of the industry. It is worth considering whether the proposed licensing committee should not take this factor into consideration in any specific case in which it is asked to grant or to refuse a license.—*Nature*.

SCIENTIFIC BOOKS

Mineralogy: An Introduction to the Study of Minerals and Crystals. By EDWARD H. KRAUS AND WALTER F. HUNT. McGraw-Hill Book Co., New York. 1920. 561 pages, about 700 figures.

When a new book enters a field supposed to be already rather thoroughly covered, the first thing that will be inquired about it is, wherein does it differ from previous books? A hasty glance through the present volume yields one answer: in the character and quality of the illustrations. The usual line-drawings of crystals are abundantly supplemented

by half-tone views of crystal models, which enable the reader to gain an unusually good idea of the shapes of the crystals described. Then there are portraits of leaders in mineralogy and allied sciences, both past and present, and representing various nationalities. And, finally, there are numerous photographs of mineral specimens, bringing out typical features of the 150 mineral species covered.

Other noteworthy features are a readable chapter on the polarizing microscope, one on gems and precious stones, and one in which the minerals are classified according to elements present, and their uses are discussed. The last 150 pages of the book are devoted to an elaborate determinative table, based on physical properties. Every effort has been made to bring out the practical side of the subject, to show wherein the facts given bear on the everyday experiences of the reader, and to make the subject matter interesting as well as informing.

In certain respects, moreover, the book is more up-to-date than is usual in an introductory text. For instance, in the definition of a mineral, allowance is made for recent discoveries as to variability in composition, and for the occurrence of colloid minerals, thus: "A mineral is a substance occurring in nature with a *characteristic* chemical composition, and *usually* possessing a definite crystalline structure. . . ." Further, a table is furnished for the use of the Merwin color screen in identifying elements by flame tests; and special tests to distinguish calcite from aragonite and from dolomite are given. Modernized formulas are listed for pyrrhotite, limonite, and bornite.

The make-up of the book is on the whole good. The crystal models would have shown up better if they had been coated with ammonium chloride before photographing. There are a number of places in which the type has evidently become pried after the last proof was corrected, but these can be readily set right on reprinting. Through a change in the vowel in the last syllable, the birthplace of scientific mineralogy appears as a castle, rather than the more appropriate mountain; microcosmic

salt becomes microscopic in one place; while phosphorus at least three times shows its affinity for o by taking up this letter into its last syllable; but all of these are changes which occur frequently in the composing room, and are of minor importance. The reviewer would prefer the Latin to the hybrid spelling of sulfur, the name columbium to niobium throughout, and diatomaceous to infusorial earth (since there are no infusoria in it). He also does not believe that classification of minerals by their metals is less scientific than by their non-metals; but that every one does not agree on such matters is an advantage to science, and not a detriment to this book.

To sum up: Because of the excellent illustrations, the up-to-dateness, and the practical nature of the information furnished, there would seem to be room for this "Mineralogy" even in a somewhat crowded field.

EDGAR T. WHERRY

U. S. DEPARTMENT OF AGRICULTURE

SPECIAL ARTICLES

ACID PRODUCTION BY A NEW SULFUR-OXIDIZING BACTERIUM

In a series of investigations on the oxidation of sulfur, which resulted in the isolation of a very strong sulfur-oxidizing bacterium, a striking fact has presented itself, namely, an intense oxidation of sulfur to sulfuric acid and a large accumulation of acids, even in the absence of neutralizing substances.

The organism is autotrophic, *i.e.*, is able to derive its energy not from the decomposition of organic substances, but from the oxidation of sulfur, although the presence of organic substances is not detrimental to its activities. The carbon, necessary for the building up of its body substances, is derived from carbon dioxide of the air. In a medium entirely free from any traces of organic materials and carbonates and containing ammonium salts as sources of nitrogen and some inorganic minerals, the organism rapidly oxidizes sulfur into sulfuric acid; the latter acts upon neutralizing substances present in the medium (tricalcium-phosphate has been used chiefly)

transforming them into salts and acid salts; when all the neutralizing substances present have been used up, free acids begin to accumulate.

Free acidity was measured both by titration, using phenolphthalein as an indicator, and by the determination of the concentration of hydrogen ions, using the phenolsulfonephthalein series of indicators added to buffer solutions. For the determination of the highly acid solutions, tropaeolin 00, methyl-violet and mauvein were used and the results checked up by the electrometric method.

The following table is typical of the acid accumulation by the organism:

TABLE I

Age of Culture	P _H	Titration. C.e. of N/10 Alkali Required to Neutralize 1 C.e. of Culture
At start	5.6	0.16
33 days	2.2	1.25
61 days	1.8	2.25
85 days	0.58	4.00

The titration does not give a true indication of the true acidity of the medium, and, although the culture, when 83 days old, was equivalent to 0.4 N acid by titration, the presence of large amounts of soluble phosphates in the medium would tend to diminish the actual free acids in the medium. But the P_H value gives a true indication of the acid concentration of the medium. The highest concentration of acid ever reported for a living phenomenon was the production of citric acid by *Aspergillus niger*, which reaches a P_H equivalent to 2.0-1.8 (Clark and Lubs¹). The acidity produced by this sulfur-oxidizing organism, as expressed in terms of P_H—0.58—is greater than that of any acidity ever reported for biologic phenomena.

A detailed study on the sulfur oxidation by this organism will soon be published in *Soil Science*.

SELMAN A. WAKSMAN,
JACOB S. JOFFE

N. J. AGRICULTURAL EXPERIMENT STATION

¹ W. M. Clark and H. A. Lubs, *J. Bact.*, 2, 1917, 1-34, 109-136, 191-236.

THE AMERICAN CHEMICAL SOCIETY

(Continued)

ORGANIC DIVISION

E. Emmet Reid, *chairman*Roger Adams, *secretary*

The oxidation of propylene glycol by means of alkaline potassium permanganate: W. L. EVANS, J. E. DAY and W. R. STEMEN.

The oxidation of isopropyl alcohol and acetone by means of alkaline potassium permanganate: W. L. EVANS and LILY BELL SEFTON.

The influence of alkali on the formation of vinyl alcohol from acetaldehyde: W. L. EVANS and C. D. LOOKER.

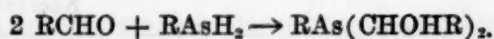
The solubility of dichloro-diethyl-sulfide in petroleum hydrocarbons and its purification by extraction with these solvents: THOMAS G. THOMPSON and HENRY O'DEEN.

Rearrangement of unsaturated acids: OLIVER KAMM and M. E. DREYFUS.

The reaction velocity of dealkylation of tertiary amines with acyl halides: OLIVER KAMM and W. F. DAY.

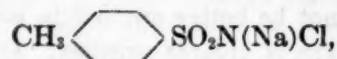
The alcoholysis of esters with amino alcohols: RUFUS M. KAMM.

Reactions of the arsines. Condensation of primary arsines with aldehydes: ROGER ADAMS and CHARLES SHATTUCK PALMER. Aromatic aldehydes and aliphatic aldehydes readily condense with phenyl arsine, when a few drops of hydrochloric acid are present, to give products consisting of two molecules of aldehyde and one of phenyl arsine. These substances are stable to water, dilute alkali and acid, and are probably represented by the structural formula given in the following equation:

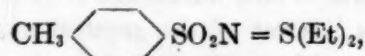


On the non-identity of α -eleostearic acid from tung oil with ordinary linolic acid: BEN. H. NICOLET. α -eleostearic acid is readily prepared from tung oil (China wood oil). On bromination in glacial acetic acid it is known to form a tetrabromide m. 115° which Lenkowsch ("Oils, Fats and Waxes," Vol. I.) suggests is identical with linolic acid tetrabromide, m. 114°. A mixed melting point showed a lowering of 15°, so that the two are obviously different. Bromination of the eleostearic acid in ligroin leads to the formation of a dibromide, with altogether different properties.

A new type of nitrogen-sulfur compounds; the action of chloramine-T on organic sulfides: BEN. H. NICOLET and IMOGENE D. WILLARD. On boiling together in alcoholic solutions diethylsulfide $(\text{C}_2\text{H}_5)_2\text{S}$ and chloramine-T,



give NaCl and a compound which is probably



since it is hydrolyzed to give p-toluenesulfonamide, and a product which on reduction gives diethylsulfide and which is presumed to be diethylsulfoxide. The reaction is believed to be rather general. Compounds containing N and S linked by a double bond, have been practically unknown.

Report on the progress of the manufacture of research organic chemicals: HANS T. CLARKE. The present report covers the work of this department of the Eastman Kodak Company during its second year of activity. As was to be anticipated, the progress made has been very much greater than during the first year as regards both the number of chemicals available and quantities distributed. At the present time nearly 800 different chemicals are available, almost all of these being organic, the balance consisting of certain inorganic chemicals employed principally in organic work. Of these 800 substances, about 600 have been prepared in our laboratory, some by purification of materials technically available, but the majority by synthesis. Over 600 different preparations have been undertaken, almost all of which have ultimately been successful. In a certain number of instances more than one product is obtained, either as a by-product or as an intermediate stage. A good deal of time is naturally now being spent upon the renewal of depleted stocks by methods which have already been developed in the laboratory, but the preparation of new compounds is still regarded as being a most important part of our work. Between 10 and 20 new chemicals are added to the list every month, and these are announced in the advertising columns of certain of the scientific periodicals. A file is kept of the names of materials for which inquiry is made, and this is constantly before us in the selection of new preparations. As soon as any chemical for which such inquiry has been made is available, the fact is made known to the party from whom the inquiry was received. It is in many cases difficult to decide whether or not a specific chemical should or should

not be prepared. A large number of inquiries are received for chemicals which we could never hope to furnish; in some instances, the preparations could be undertaken, but it is questionable whether the time devoted to working out the method and preparing a stock might not be better applied to some problem for which there is greater urgency. Our desire is to serve the research chemists of the United States, but to do this to best advantage it is necessary to consider the interest of the greatest number. We acknowledge with gratitude the continued support of the chemical manufacturers, who have supplied us not only with their regular products, but often with those which are available in quantities too small to place on the open market. The amount of chemicals sold continues to increase slowly but steadily, and the department is now almost self-supporting. It is at present being transferred to new laboratories especially designed and erected for the work, and it is expected that greater efficiency will be possible than in the improvised laboratory where the work was begun.

The production of benzoic acid and benzophenone from benzene and phosgene: ROBERT E. WILSON and EVERETT W. FULLER.

The nature of the reactions of anilines upon nitrosophenol: CARLETON E. CURRAN and C. E. BOORD. Experimental evidence shows that the first reaction product between aniline and nitrosophenol is quinone phenylhydrazone. Dilution or neutralization of the reaction mixture converts this substance into its tautomer phenyl-azophenol. Subsequent action of aniline upon the quinone-phenylhydrazone converts it into mono-anilino quinone-phenylhydrazone, dianilino quinone and azophenine, in turn. The theory is proposed that the formation of indamines by the action of anilines upon nitrosophenol is due to the semidine rearrangement of quinone-phenylhydrazones.

Reduction of polynitrophenols by hydrogen sulphide in the presence of ammonia: L. CHAS. RAI-FORD. In the preparation of starting material with which to test further the migration of acyl noted in a previous paper (*Jour. Am. Chem. Soc.*, 41, 2068 (1919)), with a view to determining the effect of acid-forming substituents in the aminophenol 2, 4-dinitrophenol was reduced by hydrogen sulphide in the presence of ammonia in the usual way. Contrary to what has heretofore been reported, isomeric substances were obtained. Work is in progress to determine the effect of other substituents (compare Anschutz und Heusler, *Ber.*, 19, 2161 (1886)).

The action of ammonia and substituted amines on allophanic ester: F. B. DAINS and E. WERTHEIM.

Hydrazoisopropane: H. L. LOCHTE and J. R. BAILEY.

A convenient method for preparing certain bromohydrins: J. B. CONANT and E. L. JACKSON.

Addition reactions involving an increase in valence of a single atom: J. B. CONANT.

New derivations of thymol and carvacrol: D. S. L. SHERK and EDWARD KREMERS. The quinhydrone hypothesis of plant pigments, as it grew out of the biochemistry of the Monardas, necessitated a revision of the underlying compounds. This study has been continued, especially along the line of intramolecular changes such as manifest themselves in connection with the nitroso compounds of the above mentioned phenols and their isonitroso rearrangement products.

The action of amines upon thymoquinone: NELLIE A. WAKEMAN and HARLAN G. GROFFMAN. Dimethylamidothymoquinone, prepared according to Zincke, yields a platonic chloride double salt containing 41 per cent. of platinum, corresponding to the union of one molecule of the base with two of acid platonic chloride. Thymoquinone treated with benzylamine, in alcoholic solution, yields dibenzylaminothymoquinone, with some mono-benzylaminothymoquinone. Thymoquinone with aniline, also with p-toluidine, under the same conditions, yields the di-derivative. No mono-derivatives have been isolated here. Thymoquinone with piperidine, under the same conditions, yields a pale purple crystalline derivative, the constitution of which has not yet been determined.

Organic mercury compounds of phenol: FRANK C. WHITMORE and E. B. MIDDLETON.

CHARLES L. PARSONS,
Secretary

SCIENCE

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